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ABSTRACT

A personal understanding of the concepts of scholarship, knowledge, and research is presented. The basic task of scholarship is to study reality with a view to finding out what it is like, the relations of different parts to each other, and the applications to which the findings can lead. The scholarly search must be a rational procedure. Scholarship can result in knowledge about tangible reality and rational ways of attaining certain goals, but it cannot solve or explain existential problems that are at the core of human life (such as belief, love, and emotion). Three types of scholarly work are evident: (1) fact finding; (2) formation of theories and hypotheses; and (3) interpretation of texts and occurrences (hermeneutics). Metaphysical, non-rational ideas, assumptions, beliefs, and convictions cannot be part of scholarly studies, although they can be analyzed and illuminated in a scholarly way. Objectivity is a basic requirement in this understanding of science, research, and scholarship. A 56-item list of references is included. (SLD)

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ON THE RATIONALE, TYPOLOGY AND METHODOLOGY OF RESEARCH AND SCHOLARSHIP

A PRACTITIONER'S UNDERSTANDING OF EPISTEMOLOGY

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Content

1 Introduction

This is a very personal presentation, an account of considerations of research principles given after many years of scholarly work. It is quite evident to me that the epistemological position on which I have based my work was long more implicit than explicitly thought out. I am convinced that this applies to a great number, perhaps a majority, of researchers in various disciplines. I was interested to see that, at the end of a Swedish book on dialectics, hermeneutics, positivism and other schools of thought in social science, Stig Lindholm writes:

> My understanding of science and scholarship is less clear and less finished today than it was ten years ago.
>
> (Lindholm 1979 p. 211)

Others are less humble. Self-confident and categorical claims with doubtful factual or logical background also occur, for example in the often repeated statement that scholarly rationality and endeavours to be objective in research harm the weak and underprivileged. It looks as though Lindholm - like me - has found it necessary to clarify his own position after years of scholarly work.

It is in the belief that various approaches to scholarship and research are felt to be important matters of discussion outside the circle of professional epistemologists and philosophers that I have decided to publish an attempt to analyse my understanding of these concepts, sometimes overarchingly called science[1], and the considerations behind it. Readers may find it profitable to compare this with their own thinking, their experiences of various

1 I avoid the word in this general sense as science is usually understood to mean natural science unless preceded by a qualifier (social science, e.g.). This by no means represents a late change of meaning. Cf. the following quotation from a periodical of 1867: 'We shall ... use the word "science" in the sense which Englishmen so commonly give to it; as expressing physical and experimental science ...'(NED Vol. VIII, s-see-eel, of 1911 p. 222). It seems regrettable that in English there is no such general term as the German word 'Wissenschaft'. Cf. Snow who by science exclusively means natural science and claims that it always embodies earlier findings. 'This ability to incorporate the past gives the sharpest diagnostic tool, if one asks whether a body of knowledge is a science or not. Do present practitioners have to go back to an original work of the past? Or has it been incorporated? The English definition of science has always been stricter thant that of Wissenschaft or nauk, and has in effect employed precisely that diagnostic tool' (Snow 1971 p. 95). On Snow's understanding of science see further under 4.3 below.

disciplines and the positions they have come to. I should be glad if a critical perusal of what I write could cause a debate among practitioners of research. The concerns I am writing about seem to me to be important, not only to philosophers but to all scholars.

My own background is experience of research work in two far from similar disciplines, linguistics and education, the former mainly in the form of diachronic studies of English phonology, the latter largely as on the one hand theory building and hypothetico-deductive testing, on the other hand applied and technologically oriented work. I am afraid that to be concrete I shall have to refer to my own experiences of research in several contexts below. This will explain the otherwise somewhat unseemly quantity of Holmberg publications listed and commented on.

Much of what has been said by recognised authorities of various competing schools on epistemology has seemed irrelevant to my research, although I have paid some attention to the epistemological aspects of the debate in the discipline of education (Holmberg 1982a-b and 1986). It is my impression that many scholars have found general discussions about methods for creating knowledge to be of little interest as often they are neither specific nor action-oriented enough to engage them.

There can in any case be no doubt that, often enough, scholars work in the research tradition in which they have been socialised without paying much attention to different approaches. Logical empiricism was thus long taken for granted as the proper approach not only in natural science but also, for instance, in social science. Karl Popper's rationalist criticism of positivism, though often misunderstood, has led to a re-appraisal of what scholarly search is and to important modifications in research relying on empirical evidence. However, Kuhn's by now classical study of 'normal science' vs scientific revolutions has, whether its conclusions are accepted or rejected, caused many scholars to consider what 'paradigm' is really theirs. Nevertheless, many scholars seem to be entirely unconcerned about the basic

assumptions underlying their work. It may be healthy if epistemological approaches are discussed in a way to cause some uncertainty and, possibly, even willingness to reconsider positions.

What has been said so far is my excuse for writing this paper.

2 The concepts of scholarship, knowledge and research

Although with some hesitation and fear of lacking exactitude I use the word scholarship in the sense of 'Wissenschaft', i.e. science not limited to natural science but meaning study and the outcome of study 'which is concerned either with a connected body of demonstrated truths or with observed facts systematically classified and more or less colligated by being brought under general laws, and which includes trustworthy methods for the discovery of new truth within its domain' (NED VIII, S - See-eel p. 221). Cf footnote 1. This is how knowledge in the sense of awareness of facts and certain understanding, as distinct from opinion, is created.

These descriptions may be used as working definitions. What we are concerned with are on the one hand systems of propositions logically related to one another, on the other hand the activities by means of which knowledge is acquired or created (Brezinka 1978 p. 31). The concept of knowledge has been taken to include various conceptions depending on the stages of development typical of the periods in which it has been searched for, i.e. the stages of scholarly or scientific development. What we now consider superstition used to be regarded as knowledge by large groups of educated people.

Today most people and presumably all scholars are likely to view knowledge as something largely attained, produced or corroborated by scholarship. Research is to me organised search for facts and solutions, theorising, theory testing and interpretation of findings (texts and occurrences). In this presentation I use the wider concept of scholarship also as a synonym of research, which I regard as the gist of scholarship. Defining the concepts of

scholarship (science, research) and knowledge, as attempted, appears necessary, but really does not help us much. We must look into the activities, methods, motives and propelling powers of scholarly work.

3 Scholarship - a rational activity

The basic task of scholarship is evidently to study reality with a view to finding out what it is like, what the relations of its different parts are to one another etc. and what possible applications its findings can lead to. This is tantamount to describing its tasks as searching for truth. Search for truth is necessarily based on a concept of knowledge as something objective. The difficulties of this search are often tremendous, as explained in the following quotation:

> The status of truth in the objective sense, as correspondence to the facts, and its role as a regulative principle, may be compared to that of a mountain peak usually wrapped in clouds. A climber may not merely have difficulties in getting there - he may not know when he gets there, because he may be unable to distinguish, in the clouds, between the main summit and a subsidiary peak. Yet this does not affect the objective existence of the summit ..... The very idea of error, or doubt (in its normal straightforward sense) implies the idea of an objective truth which we may fail to reach.
>
> (Popper 1963 p. 226)

While absolute truth is out of reach we can - and constantly do - develop and try out hypotheses which we accept as long as they have not been proved wrong. Our search proceeds, as Popper says, by conjectures and refutations.

There is no doubt in my mind that scholarly search must - once it has started - be a rational procedure. This does not mean that the choice of research areas is necessarily based on rational thinking, nor that ideas, assumptions or overarching theories emerge as a result of rationality. On the contrary, inspiration in these respects usually seems to have emotional and intuitional

backgrounds. The scholarly work that tests propositions and modifies theoretical approaches has to be intersubjectively rational, exact and of a non-partisan character, however. If it is not, the outcome can claim no scholarly value.

What I thus (and, I believe, a vast number of scholars in various disciplines) choose to call scholarship (or science) is different and by definition separate from religious knowledge, value-laden beliefs and promotion of pre-conceived convictions. Thus, to be concrete, theology as a discipline, i.e. scholarly theological research, is not concerned with preaching Christianity or promoting belief but with studying the phenomena, expressions and conditions of religious belief (exegetics, history, comparative religion etc). Detecting the basic differences between on the one hand scholarship and rational knowledge, on the other hand transcendent, religious insight, for the latter of which I have always had profound respect, was one of the great experiences of my youth. It was, of course, Kant's epistemology that made me understand that metaphysics cannot possibly be a science about a reality that is independent of our consciousness.

This view of scholarship actually implies attributing only limited importance to it. It can result in knowledge about tangible reality and about rational ways to attain certain goals (what in German is called Zweckrationalität), but it cannot solve or explain existential problems such as the meaning of life or the longings of what we call the soul. Belief, love and emotion, which are at the core of human life, are areas in which scholarship can give little or no help. Religion as such is untouched by what I consider scholarship.

My view of scholarship as described in the proceding paragraphs is evidently a definition with the character of a value judgement[2] and can as such in principle claim no scholarly status. However, it is important to see that, as shown by Runciman, there are two kinds of value judgements, one concerned with academic values, the other with political values.

2 Cf. Albert, who stresses that it is a moral rather than intellectual decision to approach all problems, as far as this is possible, with rational methods (Albert 1960 pp. 231 - 232).

> Where claims of accuracy and validity are made, whether in the natural or the social sciences, they can be overturned only by reference to criteria which are internal to the practice of science as such. Its is not an argument against accepting a scientific explanation as valid to say that accepting it as valid implies a commitment to the value of validity...
>
> (Runciman 1983 p. 43)

Like Runciman I find it obvious that 'the relation of academic to political values ... is contingent, not necessary' (op. cit.p.55).

> The values of science are not political values unless scientists make them so. A sociologist who publishes findings which convincingly support the hypothesis that inherited differences in measured intelligence vary significantly according to sex and/or race may - depending on your political values - be a bad citizen although a good scientist, just as may have been, in your eyes, the physicists who made the atomic bomb.
>
> (Runciman 1983 p. 44).

Others may define scholarship in a way that differs from my views - and have, indeed, done so. Galtung, who includes practice meant to change reality, says that also politics is scholarship (Galtung 1977 p. 64), a statement I categorically reject. If politics is given the status of scholarship, then all kinds of persuasion and indoctrination, tested only by the interests of a party or of an organisation or by the agreement with a belief or ideology, will have to be included. This would mean depriving scholarship and science of meaning - they would be empty terms only. Thus I find it important to insist on non-partisan rationality as the basic characteristic of scholarship.

'Value freedom'(Wertfreiheit) is to me an essential characteristic of the outcome of scholarly work. This concept, as introduced by Max Weber, is summarised as follows by Lessnoff 1974:

> Weber maintains two positions, one a matter of metaphysics and logic, one a practical prescription. First, there exist two distinct and separate spheres, the sphere of facts and the sphere of values. Correspondingly, there is a logical disjunction between statements of fact and statements expressing evaluations. Only the sphere of facts is the subject-matter of science, whether physical or social, for only facts, and not values, are ascertainable by the observational methods of science; science consists of statements of fact, not statements of value. Problems relating to the sphere of facts include: what phenomena exist in the world? what law-like relations hold between them; what explains them? By contrast, value-judgements are judgements of 'the satisfactory or unsatisfactory character of phenomena', of their 'desirability or undesirability'. The sphere of values includes all problems as to what should be done in a given situation, and what states of affairs one should try to bring about - problems of rightness and goodness.
>
> Weber's prescription is that the logical disjunction of facts and values should not be concealed or blurred. Anyone who expresses a value-judgement should not pretend that it is scientifically warranted, since it cannot be. If social scientists make recommendations for action, they should be at pains to make clear that every such recommendation implies some extra-scientific evaluation, whatever scientifically established facts it also rests on - for laymen may not appreciate this. As Weber notes, his precept itself depends on a value judgement: given the logical disjunction of fact and value, it follows from the principle of intellectual honesty.
>
> (Lessnoff 1974 pp. 131 - 132)

The reason why I stress my adherence to rationality in research is that reservations in this respect are in vogue among some social scientists who stress 'contextualisation' and the indivisibility of scientific and non-scientific claims; they feel that 'the vision of science as a specialized, objective non-valuation domain is undermined' (Beyer 1988 p. 73, nd that distictions 'between normative and empirical theory, fact and value, theory and practice, descriptions and evaluation can mislead and possibly distort the scientific enterprise' (Popkewitz 198' p. 53). This thinking, which in the fields where it could be applicable implies the rejection of the possibility of real scholarship, seldom occurs among representatives of disciplines little concerned with political issues, such as natural science, medicine and linguistics.

Even scholars with no wish to introduce their own religious or political views in the science concept have found it difficult to accept this limited role and function of scholarly work. Some have tried to widen it. One attempt that appears attractice has been developed in a German study by Helmut Lehner 1987, who, while querying the view that rational knowledge is based on universally valid truths whereas mystical experience is only subjective, claims that both rational man and the mystic refer to empirical evidence. He shows that what is considered rational knowledge provides a picture of competing schools of thought, which sometimes mutually exclude each other's validity; in the cases when on the whole there seems to be unanimity between scholars (as in physics) this, according to Lehner, is due to tacit acceptance of a common 'frame of thought' ('Denkrahmen') (Lehner 1987 p. 159). Lehner underlines the uncertainty of scientific findings and finds no great difference between the claims of rational scholarship and those of mysticism. Here I cannot follow him although I find his presentation fascinating and thought-provoking.

It is evidently difficult to accept that we have to count with two types of insight and understanding, one concerned with the transcendent and emotional and another one that is rational and testable. I am convinced we have to concede this dualism, limit the sphere of scholarship to the rational and thus accept the limited power that science and scholarship have in search for ultimate truth.

A parallel to this dichotomy occurs in some discussions about the functions of seats of learning. The question raised in this context is whether universities should serve general social aims, necessarily based on values, or wholly concentrate on (the exclusive value of) scholarship. Interestingly enough the Guatemalan Universidad Francisco Marroquin, for example, which seems to have close connections with the Catholic Church and is committed to many features of 'Greco-Christian philosophy of man and life' as well as to 'an education that guarantees the free analysis and discussion of diverse ideas and values', has come down decisively on the side of 'pure' scholarship. In a policy document it declares: 'The moral responsibility of universities

does not go beyond cultivating the love for the search for truth and for academic freedom'. It is the policy of this university to reject 'the confusion of political and academic categories': '... if one insists that it is the direct or indirect function of universities to concern themselves with the solution of social problems, one is saying, though not explicitly, that the function of the university is political besides being academic' (Philosophy ... p. 10, 11, 12 and 14).

4 Types of scholarly work

In epistemological literature mainly two types of scholarly work are described and discussed, on the one hand the testing of hypotheses with a view to explaining and predicting, on the other hand interpretation to serve understanding. There is an even more basic research method, however, which must be commented on, and that is simple search for facts. Let us look at these three methods.

4.1 Fact finding

An anatomist investigates in order to describe human or animal organs. Sometimes on the basis of experience he/she expects (hypothesises) certain findings, but this is far from always the case. If the search is selective it is usually theory-laden, but in other cases it is not. An anatomist may, for example, investigate certain organs of different animals without any pre-conceived notion at all, just to be able to deliver reliable descriptions. A botanist studying the flora of an area can be in a similar situation.

Parallels can be found in many areas of research. In research on education such fact finding occurs as to the characteristics of student bodies, the use of methods and media, the assessment of educational outcomes, for example. Let me further refer to something of a somewhat different kind that also comes to my mind when I think of my own work. Study of English phonology shows that

Middle English[3] a became three separate vowel qualities in Early Modern English, which cannot originally have been hypothesised. Analyses of later sources, among them orthoepists, show that from the early 18th century a further, fourth equivalent of Middle English a appeared so that since then British English has the four broad vowel qualities corresponding to Middle English a heard in, for example, made, mad, call, hard (the last one being the one occurring for the first time as late as the 18th century).[4] These facts are of interest to linguists and they have been arrived at by systematic search. It is true there is an element of interpretation in this fact finding, viz as far as the phonetic descriptions are concerned, but otherwise it can simply be regarded as counting vowels described.

The reason why this counting is done, i.e. the research-instigating factor, is of a theoretical character; while studies of this kind are not based on specified hypotheses, there is, of course, an assumption behind it that the search is worth while. So there is no denying that there is a kind of theory in the background. Nevertheless, what we have here is basically a fact-finding exercise.

While writing this I come across an explicit confirmation of the role of pure fact finding in a recent paper on the London school of phonology:

> It is essential to any branch of science and learning, both to its process of discovery and to its process of presentation, that it is explicitly based on facts. To the London School the ultimate fact is what Firth (1964:30) calls "the individual speech act", i.e. speech as actually produced and as perceived by the human senses, either direct by listening (auditory facts), or by the observation of the speech organs (physiological facts), or indirect by the recording and observation of the effects on the molecules in the channel (acoustic facts).
>
> (Stålhane Andrésen 1989 p. 2)

3 Middle English = the English spoken and written from about 1100 A.D. to at least the end of the 14th century.

4 In a book of 1956 analysing evidence provided by the Scots orthoepist James Douglas, pp. 39 - 47, I could show that the fourth a occurred about 1740 (then pronounced with a short front a similar to that in the French preposition à), and in another one of 1964 I presented a number of research results of a similar character.

Some scholars are inclined to think of all research in terms of hypotheses and testing. They may look upon my example in a different way and suggest that someone studying thirteenth-century evidence may, on the basis of today's pronunciation, hypothesise four a-vowels and, on finding only one, conclude that this hypothesis has been falsified. They may further feel that when I found the fourth a-vowel in Douglas c. 1740 I either did this on the basis of the same hypothesis for the eighteenth century and found it corroborated or that, relying on other early-eighteenth century documents, I adopted the three-vowel hypothesis and could falsify it. These would be artificial explanations, however, as analysis with a view to describing was and is the sole object of research of this type.

Accepting this kind of fact finding in research naturally implies the assumption that there is, in fact, a reality that can be observed and described objectively, i.e. inter-subjectively. Anyone checking my 18th century source (James Douglas) will find that he described four English a-vowels. This must be said as it is constantly claimed that no neutral observation is possible: All facts are supposed to be dependent on a theory.[5] This is, of course, acceptable in the sense that we have no logical right to claim that the human senses reveal 'real' objects, qualities and occurrences. But within the limits of human capacity to observe there are evidently firm facts in the sense that all human beings, when observing deliberately, perceive things in the same way.

It would be sophistic to claim otherwise. We can state as a fact that an object is a certain number of inches thick, that a process took a certain number of minutes etc. An entirely different matter is that, when not measuring, we may get very different impressions of, for example, the size of objects. Somebody driving quickly past a crossing minor road may find it narrower than somebody

5 Those who claim this are in very good company. Cf. Goethe in "Wilhelm Meisters Wanderjahre": 'Das Höchste wäre zu begreifen, daß alles Faktische schon Theorie ist.'

standing watching it. This does not make the measuring uncertain, however.[6]

It is thus unavoidable that we accept as real what can be observed. This is the only tenable paradigm if the rationality of scholarship as opposed to superstition, whitchcraft, indoctrination etc. is accepted. This is not to deny that different people may experience things differently for emotional or even physiological reasons (colourblindness is a case in point). What it means is simply that objective observation and measuring is possible in an intersubjective sense so that all observers come to the same result. While this is normally the case modern physics can evidently provide examples of situations in which observation seems to influence phenomena (cf.Heisenberg 1989 p. 83 and Leggett 1987 p.27).[7]

I wish to stress that what I have said about fact finding, does not imply an acceptance of inductive logic. No conclusions are drawn from individual examples to what applies without exception; nor do I claim that search for facts occurs independently of theoretical considerations.

The difference between induction and deduction is not always entirely clear. In some cases it may, as I have pointed out elsewhere (Holmberg 1978 pp. 4 - 5), even be rather illusory. The

6 This is to me simply common sense, but relies as a philosophical conclusion on Kant, according to whom perception is not an illusion but represents facts as they can be grasped by our forms of knowledge. This is, at least, how I have understood Kant. I find myself supported by Rossmann 1983: 'Sinnlichkeit, die reinen Anschauungsformen von Raum und Zeit, und Verstand, die reinen Begriffsformen, sind die beiden apriorisch unserem Erkenntnisvermögen selber innewohnenden Grundgegebenheiten, hinter die wir nicht zurückfragen können' (p. 272).

7 There has been much discussion of the so-called quantum measurement paradox inherent in the statement 'that in the quantum formalism things do not "happen", while in everday experience they do' (Leggett 1987 p. 169). Commenting on a 'many-worlds interpretation' Leggett refers to 'a series of formal theorems in quantum measurement theory which guarantee that the probability of two different "measurements" of the same property yielding different results (of course, under appropriately specified conditions) is zero. Crudely speaking, the probability of my hearing counter 1 click at the passage of a particular electron while you hear counter 2 click is zero. The "many-worlds" interpretation, at least as presented by its more enthusiastic advocates, then claims, again crudely speaking, that our impression that we get a particular result on each experiment is an illusion; that the alternative possible outcomes continue to exist as "parallel worlds", but that we are guaranteed, through the above formal theorems, never to be conscious of more than one "world", which is, moreover, guaranteed to be the same for all observers. The non-observed worlds are said to be "equally real" with the one we are conscious of experiencing. The more extreme adherents of this interpretation have drawn various conclusions for cosmology, psychology, and other disciplines which to the non-believer must seem distinctly exotic' (op.cit. p. 171).

knowledge of numerous instances of something happening, i.e. an inductive process, may cause the development of a theory from which are deduced possibly refutable hypotheses, which are then tried empirically. I allow myself to take the following example from my own work. A researcher notices again and again that what can be described as friendly communication, feelings of belonging and personal relations between students and tutors influence not only motivation but also study achievement favourably. General opinion among educators supports him in assuming that what he has induced from the cases he is aware of represents a law. A theory to this effect is developed and various hypotheses are derived from it - and, in fact, agreeing with what has been noticed in practice - are tried empirically. In other words assumptions are then deduced from the theory.

This is exactly what was done in the case of my theory of guided didactic conversation (Holmberg, Schuemer & Obermeier 1982). Statistical analyses of the results attained by empirical investigations may prove a likelihood of an hypothesis being superior to its competing counterpart at the significance level of .05 or better, say .01, or may give no support to the theory. To what extent does this procedure differ from an immediate investigation of the observations (more or less identical with the 'derived' hypotheses) from which the theory was in fact induced? Cf. the following presentation of how Noam Chomsky views grammar:

> He points out that the child is faced with the same sort of task as the linguist. He hears certain utterances, which Chomsky calls primary linguistic data. From this he has to devise a set of rules which will not only account for the sample of speech to which he is exposed but will also be capable of generating new sentences. This is the same as saying that the child has to develop a theory about the grammar of the language he is learning.
>
> (Greene 1972 p. 30)

Perhaps considerations like these constitute the background of what is called grounded theory (Glaser & Strauss 1977). It implies theory development through inductive methods in that researchers are expected to collect and analyse data within an area of study, beginning with observations of phenomena, and from there proceed to a selection of what is relevant and to deriving a theoretical approach.

The real problem here would seem to be whether the basic observations made by the researcher before the theory is worded have been subconsciously structured by his expectations or general thinking. This would not then be a case of induction proper.

In Popper's thinking the hypothesis always comes before the observation. In his dialogues with John Eccles, the distinguished neurobiologist, he finds not only logical but also physiological arguments for this, which Eccles seems to accept.[8]

4.2 Theories and hypotheses

The theory concept is problematic. In scholarly literature theory is a term used to denote different concepts. It is frequently used to refer to any systematic ordering of ideas about the phenomena of a field of inquiry (Gage 1963 p. 102). This (or simply the opposite of practice?) is also meant when chairs at, for instance, German universities are devoted to 'theory of education' (Theorie der Erziehung) or 'theory of the school' (Theorie der Schule). This vague use of the term theory can be rather confusing as in other scholarly contexts a theory means a set of hypotheses logically related to one another in explaining and predicting occurrences. The hypotheses are then of the types 'if A, then B', or 'the more/less A, the more/less B'. It is this last-mentioned use of the term theory that is applied in this paper.

Empirical data can corroborate, refute or leave unresolved hypotheses of this kind. Popper and his school of critical rationalists insist that theories cannot be proved, that we must accept permanent uncertainty and that theories to be studied

8 Thus Popper says that 'the epistemology fits together well with our present knowledge of brain physiology, so that both things mutually support each other. Of course, it is all conjectural: everything is conjectural, and we must not be dogmatic. But when you speak about the huge task before the brain physiologists in finding out more about, for example, the visual cortex (and the decoding in the visual cortex of the point action code which is delivered to the visual cortex by the retina via the optic nerve) I would suggest that a good conjecture and working hypothesis - a sweeping hypothesis - would be that all the integration processes or decoding processes are of the critical or trial-and-error type. That is to say, that each of them, so to speak, comes with its hypothesis and sees whether it works. The nerve cell which reacts to an inclined line is actually ready to fire or tries to fire; or it actually fires, and, if the matching is successful, it fires more, or better, or whatever it is. There is a difference if the action matches, or if it finds out that the action does not match' (Popper in Popper & Eccles 1977 p. 433).

deductively must be empirically refutable. The non-refuted theory is simply considered to have a higher degree of verisimilitude than its competing counterpart. Thus all theories are understood to apply ad hoc only, i.e. until a better theory emerges.

Popper is evidently right in stressing the limitations of the validity of theories. Scholarship develops and constantly new theories are introduced and tested. There is something very sound in Popper's insistence that theories and hypotheses should be subjected to severe falsification attempts so that, as he says somewhere, they can prove their mettle under fire. Scholars following these principles are not likely to keep any illusions about the validity of theories unsupported by empirical evidence.

To Popper a theory is falsified if only one case is found in which it does not apply. This may be sound reasoning, but is not very helpful when theories about the actions of human beings are tested. A general problem is if we can at all postulate cause-effect relationships when human beings with individual wills, wishes and aims are concerned. It is evidently possible only if we refrain from counting with any automatic cause-effect principle and limit theories to statements to the effect that <u>if</u> such and such a measure is taken under specific circumstances, <u>then</u> this is likely to - or will in most cases - lead to certain foreseen consequences. This can be reworded into the semblance of a nomological theory, i.e. one that is always and under all circumstances applicable unless the validity is expressly limited to specific circumstances: If X, then conditions facilitating Y will be created.

This is, of course, a much watered-down version of Popper's epistemological principles. It is so much so that it seems proper not to refer to it at all as Popperian, but only to say that the approach has been inspired by Popper's falsification principles. I have followed these guidelines in testing my own theories and have found them helpful in ensuring that the testing has been rigorous enough and that I have been prevented from glibly believing in pet assumptions. My aim has on the whole been to develop predictive theories (cf. Holmberg, Schuemer & Obermeier 1982, Holmberg & Schuemer 1989) and here again I am to some extent at variance with

Popper. While he explicitly says that the task of scholarship is on the one hand theoretical, to bring about explanations, on the other hand practical, to provide for application or technology (Popper 1972 p. 49), he also makes it perfectly clear that in his view 'the theorist's interest in explanation - that is, in discovering explanatory theories - is irreducible to the practical technological interest in the deduction of predictions' (Popper 1980 p. 61).

Causal theories are usually tested statistically in that correlations between variables in a sufficiently large number of cases are searched for, and this is a procedure I also have practical personal experience of. Here we come up against difficulties of inference. If a computed correlation coefficient is found to be significant, it gives information about the probability that there is a correlation in the population concerned only.It is uncertain to what extent conclusions can be drawn about other similar populations. As human beings are very different from one another there may be unexpected differences in the form of intervening variables also in and between groups selected for study, however careful the sampling has been. In the humanities scholars for this and other reasons must be very cautious in drawing generalising conclusions from correlation analysis and statistical inference generally (cf. Lessnoff 1974 pp. 67 - 74).

I have been fascinated with Popper's ideas, but have not found it possible to apply them more than superficially in the types of research, in education and linguistics that I have done. The situation would no doubt have been very different, had I done research in natural science. Be that as it may, only a small part of Popper - although a very important one - has influenced my practical approach to scholarly theories and their testing. Against this background I find no reason here to add to the discussion of the criticism that Popper's approaches have been subjected to. I limit myself to a reference to Johansson 1975, to Lakatos 1970 (pp. 116 - 132) on sophisticated vs naive

falsification and to Lessnoff (1974 p. 20) on statistical discorroboration instead of falsification.[9]

The great importance of the falsification approach is that it helps scholars to draw a clear line of division between what they think reality should be like and what findings actually show: assumptions about the universal validity of what are considered laws may be refuted. While some social scientists allow themselves to refer to their own values as if they were relevant in a scholarly situation (cf. Galtung above), it is in my view crucial for the credibility and respectability of research that what is presented as its outcome is based exclusively on rationality and empirical observations, not on wishful thinking. However, even if scholars have the best intentions here they have to steel themselves against almost ubiquitous temptations to favour pet ideas. This risk occurs in all kinds of research, also those which concern topics unlikely to stimulate partisanship. I have seen examples of emotional involvement of this kind in research concerns as unlikely as whether a given text is a genuinely Shakespearean one or a fake and what the vowel character of stressed u in words like 'cup' was like in eighteenth century English - and have in these two cases let myself be tempted to take a firmer stand than objective evidence can vouch for (cf. Tannenbaum 1927 and Holmberg 1957). The problem is particularly great in the social sciences and education, where political bias and various kinds of wishful thinking tend to distort scholarly observations.

This is a challenge that requires intersubjectivity and makes it vital that theories should be subjected to rigorous testing; on this point Popperian falsification attempts stand out.

9 A further difficulty is the following. If Popper's principles of deduction and falsification are adhered to, normal statistical inference does not seem to be wholly satisfactory as it is concerned with generalisations induced from collections of data, thus with induction and probability rather than with deduction and falsity. Cf. the following quotation:
'In sum, statistical "tests of significance" which operate by comparing an observed distribution with one which "might have been expected by chance" turn the basic logic of statistical manipulation of variables inside out, reversing the methodology of falsificationism, and producing a verificationist bias. From our point of view, then, such tests must be considered inappropriate for purposes of hypothesis testing and, if they have a place in our methodology, this should be limited to the preliminary stages of theory formation where...formalized routines of induction are about as appropriate as anything else!' (Ford 1975 pp. 405 - 406)

4.3 Interpretation

Hypothetico-deductive testing can contribute knowledge that is certain within the limits of contemporary scholarship. Brute data are reliable, but their implications are not always clear. They may need interpretation. Understanding research results, occurrences and human statements are - beside search for facts and theory testing, the two activities so far discussed - an inevitable scholarly concern.

It is a commonplace that a study of texts, whether historical, literary or legal, must include endeavours to understand what their real meanings and implications are. These endeavours regularly cause textual analyses, examinations of backgrounds, contexts and possible parallels. Not only Biblical texts require exegesis. Old and new texts can offer problems of interpretation. This also applies to human behaviour and its consequences.

If, as said above, the purpose of hypothetico-deductive methods is to find explanations why something occurs, the purpose of interpretation is understanding. After the great German philosopher Wilhelm Dilthey (who seems to have been influenced by a historian of the middle of the 19th century, J. G. Droysen) these two are known in the international epistemological debate under their German designations Erklären und Verstehen (cf. von Wright 1971 p. 5).

As a method of research, interpretation in the service of understanding is sometimes called by the Greek term hermeneutics[10] (the German Hermeneutik was used by Schleiermacher and Dilthey, for example, and is of fairly common occurrence in the writings of later scholars). Hermeneutics has been developed into a whole system of interpretative principles.

10 The word hermeneutics occurs in English texts as early as the first half of the 18th century, thus in Waterland 1737: 'Taking such liberties with sacred Writ, as are by no means allowable, upon any known rules of just and sober hermeneutics' (New English Dictionary 1899:III, Part I, Vol. V, p.243).

These include linguistic understanding, philological methodology and sometimes even search for essence.[11] Radnitzky 1970 p. 7 refers to the background of hermeneutics as interest in intersubjective mediation of participatory understanding.

Hermeneutical work is often described as being circular or spiral in that the scholar's attention moves from one part or consideration, from the individual to the general, and back again with a view to illuminating the text or occurrence under scrutiny. This is somewhat inexactly described as the hermeneutical circle. Here belong search for backgrounds and contexts, identification of the scholar's pre-conceived elements of understanding and the impact of his/her own thinking on the object of study.

Hermeneutics has been referred to as the method of the humanities, history for example, and is undoubtedly of great importance in disciplines of this kind. While explanation leading to prediction discussed above is impossible in historical subjects, it would be futile to deny that interpretation has a function in natural science and all disciplines in which texts, occurrences and data have to be understood. In my personal experience the interpretation of historical and modern documents have been decisive for phonological conclusions (Holmberg 1956, 1957, 1964 and 1965), for scholarly studies of educational principles and their practice (Holmberg 1989, e.g.) and for the exposition of the meaning of empirical data (Holmberg, Schuemer & Obermeyer 1982, e.g.). I find no reason to ascribe interpretative methods exclusively to the humanities.

In distinguishing between natural science and the humanities it seems more reasonable to speak of two kinds of understanding. This is what C.P. Snow does. He claims that science (in the sense of natural science) is cumulative and embodies its past.

> No scientist, or student of science, need ever read an original work of the past. As a general rule, he does not think of doing so. Rutherford was one of the greatest of experimental physicists, but no nuclear scientist today would study his researches of fifty years ago. Their

11 Existentialist writers have contributed to the debate (Gadamer and Heidegger, e.g.).

> substance has all been infused into the common agreement, the textbook, the contemporary papers, the living present.
>
> (Snow 1971, pp. 94 - 95)

Snow compares this with 'the humanist culture':

> Take Shakespeare and Tolstoy. Anyone partaking of the 'humanist' culture ... has to read their works as they were written. They have not passed, and cannot pass, into a general agreement or a collective mind.
>
> (ibidem p. 95)

Although what is said in the last quotation by no means applies to all kinds of scholarship described as arts subjects or humanities, the distinctions made contribute to illuminating the two types of understanding. A consideration of Snow's views further underlines the fact, that it is not always self-evident whether a discipline is to be ascribed to science or to the humanities. Phonetics is a case in point.

Affinity to hermeneutics is sometimes demonstrated in social science in a way apparently conducive to partisanship in the interest of improving society (cf. Schäfer & Schaller 1971). I dissociate myself from such approaches, but am convinced that, in the words of a well-known, far from conservative, English sociologist 'we can turn understanding to the service of scholarship' and 'think of the social sciences as striving simultaneously both to understand so as to interpret human activity, and also to achieve Martian objectivity so as to remove from it adulterating passion' (Halsey 1986 p 13). (The author refers to a Martian in the sense of a scientifically trained visitor from some other planet.)

Systematic interpretation, whether called hermeneutics or not, seems to me simply to be the outcome of common sense; it is applicable to all research areas.

5 Debate on the functions of scholarship - the domain of unbiased rationality revisited

In the preceding section I referred briefly to the Erklären-Verstehen discussion. These two approaches have been seen as dichotomic, the former representing the aim of explaining why something occurs, the latter serving interpretative purposes. The former has also been described as nomothetic, intent on generalising, the latter ideographic, describing what is unique and individual (for example in history).[12] Wilhelm Dilthey ascribed nomothetic research to the natural sciences and ideographic study to the humanities (Geisteswissenschaften). This seems to me both an unnecessary and misleading separation of methods. There is little doubt in my mind that why-explanations promote understanding and vice versa. It seems highly artificial to make either-or distinctions here as both-and approaches are evidently profitable. There is no conflict between the aims of explaining and understanding. We have reason to speak of complementarity instead.

However, in the epistemological debate the distinction referred to is very much alive and has even been expanded to include a third supposedly differing 'knowledge interest', viz. emancipation. While the natural sciences are seen as serving technical knowledge interest and the humanities a hermeneutical one, the social sciences are supposed to serve emancipation. Cf. Habermas 1968. Any rational study aims at emancipating scholarship from prejudices, preconceived but erroneous notions and misguided conclusions, so it is difficult to see why the emancipatory aim should be stressed particularly in connection with social science - unless political issues are concerned. Among most of those who preach the gospel of emancipation as inherent in the tasks of scholarship this connection is evident, however. This applies to the so-called Frankfurt School. It is typical of this school that it rejects the principle that scholarship should attempt to be neutral in relation to value judgements, i.e. it takes a stand wholly contrary to rationalism as described above. Unbiased

12 According to von Wright p. 172 these terms emanate from W. Windelband 1894.

research is supposed to contribute to conserving the status quo. Theodor Adorno compares the separation of value judgements from research with capitulation before power, Jürgen Habermas states that it deprives analytical-empirical investigations of their contact with reality (Lebensbezug) and the advocates of this approach, often in Marxist terms, stress the importance of considering society as a totality inclusive of its mechanisms of compulsion (cf. Adorno 1975, Horkheimer 1974 and - for a critical discussion - Kromphardt & Clever 1975).

This can all be related to the view that research should have as its task to change society rather than to reveal and explain reality and/or create a basis for technology which indicates how and by what means aims can be obtained. When the adjective critical or the noun criticism (as in Ideologiekritik) is used, the idea is usually that traditional, bourgeois conceptions should be criticised, whereas so-called progressive virtues and aims are taken for granted.

The Frankfurt school calls its message the 'critical theory'. A so-called pedagogy of the critical theory has been developed. It implies on the one hand criticism of capitalist society, on the other hand great appreciation of an exchange of ideas and 'communicative competence', expected under ideal circumstances to lead to consensus. Although these ideal conditions do not exist, man is expected to act as though 'the ideal speaking situation' were reality, which is assumed to imply that the better argument consistently prevails so that consensus may be attained. The regulative idea of the ideal speaking situation is supposed to legitimate educational goals and norms for action and further to reveal conditions which make it possible to query claims of validity with a view to attaining 'true consensus' (Mollenhauer 1978 2, pp. 79 - 85).

In a book extolling Habermas' contributions and the virtues of critical theory generally Carr & Kemmis 1983 p. 180 claim that the 'aim of critical social theory is the relentless criticism of existing social conditions which maintain irrationality, injustice

and social fragmentation, and domination and coercion'.[13] This can admittedly be a noble aim, but a political rather than a scholarly one. If the aim stated is accepted (on non-scholarly grounds), scholarship may indicate methods to attain it. A kind of means-ends rationality or technology may then be applied.

When doctors and the science of medicine endeavour to improve the health of individuals and health conditions generally, they are usually in the fortunate position of knowing what is meant by health and improvement. This, unfortunately, is not the case when social conditions are considered. Is it just or unjust that somebody with a highly responsible job and superior qualifications should earn three times as much as a hard-working labourer? Is it just or unjust that immigrant workers should have the vote although they are not citizens of the country concerned? Is it just or unjust that the hard-working, knowledgeable and competent should enjoy greater privileges than those less hard-working, knowledgeable and competent?

On issues like these, well informed, well meaning and responsible people hold different opinions. No one can demonstrate that he or she is right or that other people represent the 'wrong' opinions. Here we are concerned with value judgements, not with facts or explanations why or why not something occurs, and have returned to the starting-point, scholarship as a rational, unbiased activity. The content of values, like religious belief, necessarily influences individuals and the world in general, but is definitely outside the area of science and scholarship, except in the functions of instigator, conveyor of ideas and propelling-power.

To allow political or other ideological aims to legitimate value judgements as scholarly arguments gives an arbitrary character to the outcomes. This is inevitable when opinions are not separated from facts. There is - and can be - no scholarly evidence to prove that some values are 'truer' than others. Conservatives, liberals and various interpreters of socialism come to very different conclusions about what is good for society and its citizens. While

13 This alarming description does not, as could have been expected, refer to a dictatorship but to parliamentary Western society and, evidently in particular, Australia.

it is perfectly acceptable to use scholarly arguments in the political debate, no one has the right to claim scholarly or scientific status for his/her opinions and values.

Emancipation is an attractive concept also to those who refuse to regard values as scholarly arguments and are wary of the political connotations of the critical theory. It is interesting to see that Radnitzky, who is impressed both by Habermas' argumentation for emancipation as a scholarly aim and by Popper, whose 'mission seems to be that of enlightenment' (Radnitzky Vol. II p. 139), looks for signs of rapprochement between the antipoles of Popper's strict limitation of scholarship to intersubjective rationality and Habermas' commitment to social change. He wonders 'whether Popper's general position has not more affinity with Habermas' than the Habermas-Albert debate[14] seems to appreciate' (Radnitzky Vol. II p. 140).

I find it difficult to believe in this rapprochement (in spite of possible affinities between the two in a political sense) as Habermas opens up the border between rationality and metaphysics. He does not separate value judgements from scholarship, which is a sine qua non to Popper. I cannot myself see that the introduction of emancipatory thinking along Habermas' lines makes any contribution to the methods or conclusions of scholarship. However, it can - like any metaphysical idea and emotional commitment - legitimately inspire research.

14 This debate has been published in Adorno (ed.) 1975.

6 Further comments and conclusions

The above discussion can be summarised under two headings.

6.1 Rationality and its degrees of strictness vs normative approaches

Insistence on rationality in science and scholarship generally has caused scholars to lay down rules for their work. The strictest rule is no doubt Popper's, according to whom a statement must be falsifiable to be scientifically relevant; if it is not, Popper does not recognise it as scientific (scholarly). A statement is considered falsified if only one (empirical) instance is found that contradicts it. Lakatos, who, as already mentioned, distinguishes between sophisticated and naive falsificationism, is less severe than Popper:

> The main difference from Popper's original version is, I think, that in my conception criticism does not - and must not - kill as fast as Popper imagined. Purely negative, destructive criticism, like 'refutation' or demonstration of an inconsistency does not eliminate a programme. Criticism of a programme is a long and often frustrating process and one must treat budding programmes leniently.
>
> (Lakatos 1970 p. 179)

This somewhat permissive approach is undoubtedly openly or tacitly accepted by many scholars. Here again a personal admission seems to me to be called for. I am afraid I, for one, have felt my conscience a little lulled by this thinking. After a painstaking empirical study by a colleague of the possible impact of frequent communication in distance education, which gave no conclusive evidence, I initiated a parallel study based on the hypothesis (my firm conviction) that frequent communication supports students' motivation and makes for superior study results. Even after this

study had failed to support my hypothesis I found it difficult to regard it as falsified as it seemed so reasonable.[15]

Negative, unexpected or unwanted data must not be neglected; nor should logically or factually unsupported attempts be made to explain them away. It is evident, on the other hand, that refutation/falsification is nothing automatically inferrable from statistical evidence; the conditions of any study carried out, the inclusiveness of the hypotheses tested and any intervening variables must be borne in mind. Cf. Phillips 1987 p. 13:

> There is no mechanical procedure by which a given portion of a theoretical network in science can be put to decisive test. (It is here that Popper's views have run into trouble; testing and refutation are not so watertight as some of his work implies. A scientist can use a theory, or part of a theory, to deduce a prediction that if X is done, then Y should result. But if this test is carried out and Y does not result, refutation does not automatically follow, for there are many ways the new evidence can be accommodated; similarly, if Y does result, then there are various ways in which this can be accounted for. Again, a challenge for professional judgment.

Recommendable tenacity can easily be perverted into exaggerated reluctance to give up or modify scholarly positions. This is less unusual than assumed by most scholars adhering to Popper's principles. It is not necessary to turn to anarchistic thinkers like Feyerabend for examples of criticism of such rigidity. Cf. Naess' conclusions from the study of two papers by Lakatos:

15 Some further thinking made it clear that what was wrong with the hypothesis was on the one hand that it was too general, on the other hand that the uncertainty of any judgement of what is to be considered a suitable quantity of learning matter as a basis of communication is so far forbiddingly great. Some kind of operationalisation seems to be required here (cf. Holmberg & Schuemer 1989).
Much criticism has been levelled at the Popperian falsification approach (cf. Johansson 1975), but although there are serious problems connected with it I do not think there can be any doubt that its logical basis is sound (as a theory cannot be definitely proved to be true) and that it makes for rigorous testing of theories.

> The criteria of whether a theory or a research programme or a hypothesis should be abandoned (until further notice or finally, in part or in whole, by all or by some) are local in character, both in space and time, and highly tentative. There are no general rules or standards (except very trivial ones).
> ...Study of past decisions (concerning abandonment) reveals astonishing, unpredictable revivals (rehabilitations) of theories (etc.) which were universally abandoned for very good reascns in terms of Lakatos's own standards.
> ...Proliferation of theories (etc.) serves scientific growth, but one should note (temporary) successes and set-backs, and abandon them neither too early nor too late.
>
> (Naess 1972 pp. 40 - 41)

This is reasonable enough, but does not offer much guidance; awareness of the problem discussed may lead to unjustifiable benefit-of-the-doubt acceptance of falsified theories, which, of course, scholars must be on guard against. At most this awareness may justify a relativism that can cause a scholar to retain the direction of the assumptions which have had to be abandoned and modify his/her hypotheses in the light of the falsifying evidence.

Rational scholarly work principles inevitably clash with partisan approaches and tendencies to give scholarly dignity to beliefs and values held. Scholars filled with social zeal and a wish to improve society and even in their research fight for what, in my view often well-advisedly, they consider justice have tried to introduce their normative thinking into scientific methodology in a way that in principle resembles the Christian commitments of early theologians in their exposition of God's qualities and will.

As shown already by Kant, questions concerning matters of beliefs and values cannot be solved by rational means. If, as said at the beginning of this paper, rationality is taken to be the pervading characteristic of science and scholarship, then all concerns of this type have to be left out of account in the context of scholarly methodology.

Metaphysical, non-rational ideas, assumptions, beliefs and convictions can give impetus to scholarly studies, but cannot be part of them. They can also be analysed and illuminated in a

scholarly way although their bases are rooted in methaphysical assumptions or beliefs, the truth and relevance of which cannot be subjected to scholarly investigation. It is interesting to note that Wilhelm Dilthey, the great German humanist, rejects tendencies to base specific moral-educational guidelines on scholarly knowledge. By declining to accept 'ethical systems' he is supposed to have prevented a normative regulation of educational aims in a way influential 'far into the twentieth century' (Schurr 1976 according to Hoffmann 1980 p. 143).

6.2 Scholarly methods

Against this background the only methods that I can consider open to us in scholarship are (1) fact finding, (2) theory development and testing and (3) interpretation of texts and occurrences (hermeneutics). Cf 4 above.

Search for facts is an inevitable and important part of scholarly work. Facts seen and interpreted in a context, illuminated by theories and tested against them can as a rule be subsumed under inclusive systems and concepts.[16] However important facts are, it is inadvisable to try to start out from them with a view to coming to conclusions inductively. Scholarship (science) 'cannot start with observations, or with the "collection of data", as some students of method believe. Before we can collect data, our interest in data of a certain kind must be aroused; the problem always comes first. The problem in its turn may be suggested by practical needs, or by scientific or pre-scientific beliefs which, for some reason or other, appear to be in need of revision' (Popper 1961 p. 121). This need not clash with the principles of so-called grounded theory, which, as shown above under 4.1, start out from data connected with a problem to be studied.

16 Cf. Medawar 1984 p. 29: 'The factual burden of a science varies inversely with its degree of maturity. As a science advances, particular facts are comprehended within, and therefore in a sense annihilated by, general statements of steadily increasing explanatory power and compass - whereupon the facts need no longer be known explicitly, that is, spelled out and kept in mind. In all sciences we are being progressively relieved of the burden of singular instances, the tyranny of the particular. We need no longer record the fall of every apple.'

Problems give rise to theories in the sense of bodies of hypotheses logically related to one another. Deduction can be brought about by the development of hypotheses which, I believe, should be tested rigorously. The most rigorous testing known to me and referred to above consists of severe falsification attempts in Popper's spirit. 'Boldness in conjectures on the one hand and austerity in refutations on the other: this is Popper's recipe' (Lakatos 1970 p. 92), and in my view one worth applying as far as at all possible.

Factual occurrences, data emerging from hypothetico-deductive testing, historical and other texts have to be interpreted by scholars. Hermeneutical methods developed for understanding may be useful here.

Methods for fact finding, for theory building and testing of theories as well as for interpretation serving understanding are undoubtedly all applicable to the rational analyses and syntheses that are required of science and scholarly work. Beliefs, values and subjective feelings can have no scholarly or scientific arguing power. Objectivity in the sense of intersubjective testability is a basic requirement in my understanding of science, research and scholarship.

I am indebted to Dr. Helmut Lehner, University of Konstanz, and Dr. Monika Weingartz, FernUniversität, both for many years my assistants, for a series of profitable informal discussions about research methods beginning more than a decade ago, and to Dr. Rudolf Schuemer, FernUniversität, my research associate in several studies, for always doing his best to keep me on the narrow path of rational scholarly virtue.
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